

Why could not the record before us be published in England as well as in America? This seems a very feasible plan, and would doubtless add to the usefulness of the work, inasmuch as English collaborators might be added.

Very different from the American annual is the English year-book, yet it is, we believe, the only "year-book of science" of which we can boast. Outside it resembles a shilling railway novel; inside it is a pleasant gossiping account of odds and ends of science picked up at the Royal Institution. An altogether disproportionate amount of space is devoted to extracts from the papers and addresses of Prof. Tyndall, and the woodcuts on the title-page are taken from the same source. We are glad, however, that the "Year-book of Facts" still remains, notwithstanding the death of its former indefatigable compiler. Mr. Vincent tells us he undertook at short notice to continue the work of the late Mr. Timbs. To compile a year-book under such circumstances can be no light duty, and hence we must be lenient to its shortcomings. So far as the book goes, Mr. Vincent has done his work well, and gives a bill of fare that no doubt will be relished by the *dilettante* scientific public. But it should be clearly understood that the volume is merely a *scrap-book* of popular science, and not in any sense an annual register, such as we hope may soon be issued.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Systems of Consanguinity

IN Sir John Lubbock's *vindication* of his original charge that I seem to have two theories of the facts in my work on Consanguinity (*NATURE*, vol. xii. p. 124), he fails to show that the classificatory system was interpreted by me as "arbitrary, artificial, and intentional." This is one of the theories, and in fact the principal one, which he ascribes to me, and which I repudiate. The other theory, that which I did advocate, is presented both in his address before the Anthropological Institute and in this vindication (stated partially and imperfectly, as something that I "admit." "Mr. Morgan admits that systems of relationship have undergone a gradual development, following that of the social system." (Address, p. 4, *NATURE*, vol. xii. p. 125).)

It would require too free a use of your columns to explain at length how, by quotations severed from their connections, and by a use of their phraseology not in accordance with my design, a defence of an unwarranted statement has been put together.

I beg leave to re-state the propositions in my work on Consanguinity, which contain the substance of the views I have advocated, and to which I stand committed; and to request those who may be interested in the subject to read the last chapter in the light of these statements.

In that chapter, entitled "General Results," the facts are discussed under seven propositions, in substance the following:—

Proposition I. That the systems of consanguinity given in the tables may be resolved into two, which are radically distinct, one of which is called *descriptive* and the other *classificatory*. The first is that of the Aryan, Semitic and Uralian families, and the second that of the Malayan, Turanian, and Ganowanian families.

Proposition II. That these systems are to be ranked as domestic institutions.

Proposition III. (in full). "Can the origin of the descriptive system be accounted for and explained, from the nature of descents and upon the principle of natural suggestion, on the assumption of the antecedent existence of marriage between single pairs?" (Con. p. 472.)

The affirmative of this proposition is maintained. "It is the institution of marriage between single pairs which teaches the descriptive system; whilst this form of marriage has been taught by nature through the slow growth of the experience of the ages." (Con. p. 469.)

Proposition IV. (in full). "Can the origin of the classificatory

system be accounted for and explained, from the nature of descents, upon the assumption of the existence of a series of customs and institutions antecedent to a state of marriage between single pairs, of which the Hawaiian custom is one?" (Ib. p. 474.)

The affirmative of this proposition is likewise maintained. Under it the solution of the origin of the Malayan system is given, and also of the Turanian, together with the customs and institutions, fifteen in number, arranged in a sequence, which stand connected with the birth and growth of these systems. Assuming, for example, the intermarriage of brothers and sisters in a group, every relationship in the Malayan system is found to be that which would actually exist; wherefore, the system itself proves the antecedent existence of this form of marriage. The same line of argument and of inference is then applied to the Turanian system. In Propositions III. and IV. I speak of both forms as *natural* in contradistinction to *artificial*, although they are radically different. They are natural in the sense that they are in accordance with descents as they actually existed when each system respectively was formed. This is the main proposition in that chapter, occupying in its discussion nineteen of its forty-three pages. It presents the theory of the author; it is the only place where the origin of the classificatory system is discussed.

Proposition V. This proposition maintains the unity of origin of such tribes of the American aborigines as are found to possess an identical system of consanguinity.

Proposition VI. (in full). "Where two or more families, constituted independently on the basis of such a system of relationship, are found in disconnected areas or upon different continents, can their genealogical connection be legitimately inferred from their joint possession of the same system?" (Ib. p. 498.)

After showing that the people of South India who speak the Tamil, Telugu, and Canarese dialects have a system of consanguinity identical with that of the Seneca-Iroquois of New York, in upwards of 200 relationships, the question is raised, "How shall this identity be explained?" It was my discussion of this question that confounded my distinguished adversary, which he misunderstood at first, and is not sure that he "quite comprehends even now." How his difficulty could have arisen I confess puzzles me. Under Proposition III. the origin of the descriptive system had been discussed, and under Proposition IV. that of the classificatory; but under this (VI.) the question was whether any evidence of the Asiatic origin of the Ganowanian family could be found in this identity of systems. The four hypotheses quoted by him (vol. xii. p. 124) are produced and discussed here. "Spontaneous growth" was referred to and of course rejected as an adequate explanation of this identity of systems.

Proposition VII. relates to inferences that may be drawn from partial identity of systems.

These several propositions show very plainly, I submit, that these systems are not explained in that volume as "arbitrary, artificial, and intentional," and equally plainly that they are explained as growths or results of certain customs and institutions.

Turning now to Sir John Lubbock's vindication, his first principal quotation is taken from the discussion of my first proposition, where "natural and spontaneous" is used in opposition to resulting growths from customs and institutions, the cause being unknown in the first case, and known in the second. His second quotation is from the discussion of my sixth proposition, where "spontaneous growth" is used, and in the same sense.

The discussion of the mass of materials accumulated in that volume was confined to forty-three out of five hundred and eighty-three pages. It was a new subject, in which it was necessary to invent, to some extent, a new terminology. I am aware of its great defects, but I deny that two theories of the facts are to be found therein, or that I have explained the classificatory system as "arbitrary, artificial, and intentional," which is the point from which this discussion started.

Rochester, New York, July 20 LEWIS H. MORGAN

Weather on the Atlantic

I HAVE reluctantly come to the conclusion that attempts to forecast the weather on the North Atlantic frequently result in disappointment. A recent passage from New York to this country has enabled me to gather some remarkable data on this subject, so remarkable, indeed, that any one crossing for the first time might reasonably question the action of the barometer. If I had had only one on board, I should certainly have doubted its

accuracy, but having three, the readings of them cannot be questioned by the most sceptical.

We left Sandy Hook on the forenoon of April 10, with a light north-west wind and pleasant weather. Temperature of the air 46, barometer 29.82. From that date to the 16th the ship steamed 2,210 miles, and the mercury, with the exception of a slight fluctuation which never exceeded $\frac{1}{100}$ of an inch, fell steadily until it reached 29.14 on the latter day. Throughout this period the wind veered and backed between N.N.W. and E.N.E., never exceeding in force a whole sail breeze, and frequently light or calm for hours together. The sky was generally overcast until the meridian of 32° W. was passed; light rain fell once, but no snow or sleet. Temperature of the air ranging between 34° and 57°.

Until the ship was to the eastward of Cape Race (passing 300 miles south of it), as no gale blew I expected a heavy fall of snow; but as it did not come, I assumed that the snow-covered ice on the Grand Bank of Newfoundland caused this unusual depression of the mercury. Great was my surprise, therefore, to see it falling lower as the distance increased from the supposed cause of the depression, while the wind gradually died away, the clouds opened out and assumed softer forms, the horizon cleared, and the long northerly swell subsided. The latter is always a sign of fair weather on this troubled sea. If a storm be advancing towards a ship, the swell usually comes before the wind, so quickly is the motion of the water translated.

While on the subject of waves, I may state that I have been investigating the cause of the greater height of the waves raised by a north-west wind above those raised by a south wind. The observations were made while crossing several offshoots of the Gulf Stream, and I found that in every instance the sea was smoother in the warm water than in the cold. If this view be correct, then the waves in tropical seas should be inferior in height to those of the temperate zone. The question is, Are they so?

Snow has an extraordinary effect on the barometer, but its action is most mysterious, as in this case the ship was several hundred miles from any locality where snow could have fallen.

In March 1872 I witnessed a similar instance of great depression in the barometer, with no wind and a clear sky. On reaching the land it was found to be covered with snow. In that instance there was scarcely a cloud visible during the last 400 miles, and not a single flake fell on the ship. I believe the remarks of an old seaman on the weather of the Atlantic are very true, viz., "The longer one sails on it the less one knows about it."

The presence of heavy field-ice in the month of April, so far south as 41° 40' N., only fifty miles north of the latitude of Naples, has excited considerable astonishment amongst Atlantic navigators, since many steamers were entangled in it as early as the 24th of January.

The Admiralty Chart of 1873 indicates March as the first month of its arrival, and further gives lat. 42° 13' N. as the extreme southern limit of its existence, whereas it has already been met with twenty-seven miles south of that parallel, forming a dangerous barrier to ships on the great highway to America; and the commanders of those vessels, relying implicitly on the correctness of a survey which should be above suspicion, have seriously injured their vessels, thereby jeopardising many lives and valuable property in a locality where every feature of it should be as well known as the waters of the Serpentine or the Thames above bridge. It is, moreover, notorious that this is not the only defect in the chart of 1873. The northern limit of the Gulf Stream is laid down from 100 to 150 miles south of its true position; and the existence of another important current (the Labrador), which plays no mean part in the economy of the globe, is entirely ignored, although its line of demarcation from the adjacent waters is as well defined as that of its great neighbour.

It is stated by the old residents of Canada that such a severe winter as this has not occurred in the Dominion for forty years. During the months of January and February at Montreal the wind only blew from the south for six hours. Not only was the thermometer low, but the northerly gales were incessant, rendering outdoor exercise almost an impossibility. These storms broke the ice of Newfoundland and Labrador from its moorings before the summer sun could soften it, and hence the reason of its floating down south. Being almost as hard as granite, and with the sea water at 30°, it will not readily decompose.

The recent severe winter must affect the fortunes of the polar expedition for good or for evil. Channels into which ice has

drifted will become inaccessible until late in the season, but, on the other hand, the pack-ice will be less inconvenient from its solidity and compactness. It is not probable that many large bergs will reach the Atlantic this season.

Celtic

W. W. KIDDLE, R.N.

The late W. J. Henwood, F.R.S.

MR. G. T. BETTANY is no doubt very nearly, if not quite, correct in saying of Mr. Henwood (NATURE, vol. xii. p. 293), "I believe that scarcely one of his cherished objects in this respect [the arrangement of his stores of facts and observations] remains unfulfilled."

In a letter to me, dated July 31, 1875, Mr. Henwood remarked: "I believe all I have done since [I wrote you last] has been to make some preliminary calculations regarding the corrections for temperature of the results of my observations on magnetic intensity, made on the surface and near the bottom of Dolcoath Mine in 1832. I think they hold out promise of something if I have only strength to put them in order." On the fifth day after writing this he died. M. Y.

Zoology of the "Erebus" and "Terror"

Palmarum qui meruit ferat. Referring to the article on this subject (NATURE, vol. xii. p. 289), allow us, as the publishers of the botanical portion, to say that the indefatigable labours of Dr. Hooker, aided by the Government grant, resulted in six vols. 4to.; not two, as stated by the reviewer. This was published in three divisions, viz.: 1. Flora Antarctica, 2 vols.; 2. Flora Novæ Zealandæ, 2 vols.; 3. Flora Tasmaniae, 2 vols.; the whole comprising nearly 600 coloured plates.

L. REEVE AND CO.

5, Henrietta Street, Covent Garden, Aug. 14

The Rocks at Ilfracombe

COULD any of your readers state in your columns the nature of a curious appearance in the rock near Ilfracombe (North Devon), on the way to Coombe Martin, just where the road begins to descend to the latter place? Here on the right-hand side the bank is considerably excavated, and through the scaly and friable strata, whose cut surface is perpendicular to the road, rock of a harder kind seems to have been pushed, presenting a rounded surface, which gives the appearance of trees laid in the bank and partly uncovered; indeed, I first heard of them as "petrified trees," and from the road they look very much like the trunks of silver birches. Our Ilfracombe driver told me that a great many people came to look at them, some saying they were trees, others that they were not.

There are several of them, and various lengths are visible, from about a yard to twenty feet, I should think.

WILLIAM S. TUKE

OUR ASTRONOMICAL COLUMN

BINARY STARS.—(1) η CASSIOPEÆ.—Dr. Duner, of the Observatory of Lund, Sweden, has calculated elements of this binary from measures 1782-1874; the orbit is as follows:—

Peri-astron passage, 1748.413	
Angle between the lines of nodes and apsidæ 245° 9' 1"	} Meridian of 1850
Node 50° 83'	
Inclination 68° 46'	
Angle of excentricity (= $\sin^{-1} e$) 38° 8' 12"	
Mean annual motion + 2° 04' 11.2"	
Semi-axis major 10" 681	
Period of revolution 176° 374 years.	

The comparison with measures used by Dr. Duner in his calculation shows very small residual errors, but the elements here transferred from Leverrier's "Bulletin International" of the 12th inst., though representing the angles of Struve, Dawes, Jacobs, and Dembowski, with small negative errors give the distances measured since 1827, very sensibly in defect of the observations. Thus for Dembowski's measures we have—

1863.26	Error in position	— 0".72	Error in distance	— 0".69
1867.16	"	— 0".71	"	— 0".63
1871.05	"	— 0".18	"	— 0".46

For a normal founded upon measures by Jacobs, Dawes, and Dembowski, for 1854.20, the error in position